

on a large number of individuals, I cannot place on record a single instance of clear and unmistakable scorpion suicide.

Rondebosch, January 1

C. LLOYD MORGAN

Mimicry in Moths

I HAVE read with great interest the observations of the Duke of Argyll on Mimicry in Moths. I remember more than one similar occurrence during my travels. The most curious was as follows:—

Whilst in Japan, a messmate brought on board, in an ordinary po, a beautiful trained shrub with a leaf much resembling that of an orange. It was placed on the ward room table where we all sat, the steward removed it from the table to the top of an harmonium at least three times a day, and I watered it when required, and often examined and admired it; in about eight or ten days it began to show signs of failing; and, thinking it might be infected with spider or green fly, I examined it carefully, and in doing so I disturbed a large green smooth-skinned caterpillar. Like all animals on board ship he soon became a great favourite, and we often asked strangers to point him out and in no case did they succeed.

He always lay along the edge of the leaf, with his head to the point and eat at each bite, exactly the breadth necessary to preserve the contour of the leaf as far as possible, when he reached the point, by a few sharp convulsions he returned to the stem and began another row. When he had finished one half of the leaf he began the other; and when nothing but the centre rib of the leaf was left he eat backwards along the stem. He was the most economical feeder I ever saw, only a very little bit of the centre rib of the leaf was bitten off and fell to the ground, and the hard stem of the leaf was left.

I soon observed that he could assume the exact shade of the leaf he was feeding on, and I frequently shifted him and watched the process.

In due time he assumed the chrysalis form; he partly suspended, partly glued, himself to the stem of the plant and it was very difficult to detect him; but not nearly so difficult as in the caterpillar state.

He remained a very short time in the pupa, and one day I was called by a messmate who informed me that "My beastly bug had hatched out," and at first I thought this was the case, as a beautiful black and gold butterfly was expanding his wings and legs on the table, and soon took wing, but was captured and handed over to our bug collector, who by the way took no interest whatever in the prior stages; he was neither butterfly, moth, nor beetle, so nothing to him.

I went to observe how he had broken out of the sheath and was astonished to find that my chrysalis was safe and sound, the butterfly we had certainly did not come from it. Then where did it come from? We were still in Yokohama harbour, and it was a common occurrence that insects flew off to the ships. But how did a butterfly in the state I saw this one get on the ward room table? I came to the conclusion that the pupa had been attached to the plant or pot; I did not anticipate what took place. In a few days another butterfly, to all appearance the brother of the first one, was seen (but not by me), to emerge from the chrysalis we had at first observed; and I have no doubt the first insect had eluded all our prying, and that there were two caterpillars all the time on the plant.

I do not get NATURE until it is a fortnight old, and I have waited with anxiety to see if any one better able than I am would endeavour to show that mere physical causation is sufficient to account for all the phenomena disclosed by the Duke's admirable observation of the moth.

I look upon the Duke as one of the best observers of Nature we have, and his opinions must carry great weight; and believing as I do that in the Theory of Natural Selection the future existence of our race and all hope of advancement in morality is bound up, I am anxious that his doubts on this subject should not carry weight with others.

I think the whole question lies in this—were either of these caterpillars, or the Duke's moth perfect, or even the most perfect of their kind?

I believe I have had more opportunity of observing cases of mimicry than his Grace has, and I have always found that the individuals vary as much in these forms of life as in any other. At Labuan one of the Engineers of the coal works sent a native out and in half an hour he returned with seven leaf-insects. I had picked one up in my walk from the settlement, and although at first each appeared a perfect leaf to my eye, I soon found

great differences between the individuals; some being much better specimens than others—just as all sheep are not sheepish to the shepherd—and I think it is quite possible that not one of these eight insects would deceive the eye of an average natural enemy. Let us suppose that anyone of these were so perfect as a mimic, that it would deceive this enemy, it might be wanting in the advantage of perfect rest whilst under inspection, and thus be detected. It was by the movement of the insect that I was enabled to get the one I picked up. The Duke's moth was betrayed by his "beaded eyes and thorax;" and last of all, there was a small hole in the covering of the bright wings, which the Duke considers one of the mysteries of nature, and through all the mimicry of this moth the Duke with very little trouble detects the imposter; as far as he was concerned, all the effort of nature was wasted. If I may be allowed the paradox, it is only when one has come to see what a botch nature has made of its work that its beauties can be properly appreciated. I admire quite as much the quickness of eye that belongs to the lizard that may have been on the watch to capture the moth; these "mysteries" have gone on together; and where a moth or a lizard failed ever so little it went down whilst its better appointed brother was the fittest to survive. Until the mind has taken in how constant the battle is, how small the advantages must be when the enemy is travelling the same path, it is difficult to resist the feeling of wonder and the desire to account for all by a fiat of creation.

I remember some remarks by the Duke of Argyll in a similar strain, when he observed three water-oozels take the water for the first time. He was struck with the way in which they all dived and swam, so perfectly; but I think he failed to consider this view of the matter—did any one of these surpass the others in the art, even were his advantage so little that the Duke was unable to detect it? if so, then provided he was equal of his brothers in all other respects, he was the fittest to survive; and as we evolutionists only claim little by little; its ordinary phrases are no lean and empty formulæ to me.

Nothing but the conviction that, in the new light thrown on nature by Charles Darwin and his numerous disciples, lies the happiness or misery of our race, would have emboldened me, so indifferently educated for the task, to take up the subject and your time.

DUNCAN STEWART

Knockrioch, January 25

Clerk-Maxwell on Stress

CAN any of your readers give me a reference to the note in which Maxwell, commenting on or replying to a correspondent of NATURE, gave his ideas as to the nature of stress in a beam or cord?

T.

The Comet

MAY I ask space to make some observations about the orbit of the Great Comet of 1882?

Looking on the many elements published in NATURE, in the *Dunckel Circulares*, and in the *Astronomische Nachrichten*, I find very great differences between one and another. Especially the elliptical elements calculated by Mr. S. C. Chandler, Mr. Frisby, Mr. Kreutz, and Mr. Morrison present periods peculiarly different.

Now this fact can be produced but by two causes; either it may be that the different observers considered different parts of the nucleus as the brightest part; or it may be that the movement of the comet has been much perturbed by some bodies of the solar system.

The first hypothesis is very probable, as you remark in the "Astronomical Column" in NATURE, vol. xxvii. p. 300.

The division of the head in two, and perhaps three portions, is a fact well observed by many astronomers, and well shown in the drawings published by Mr. A. A. Common, Dr. Doberck, and Mr. W. T. Sampson in NATURE, vol. xxvii. pp. 109, 129, and 150.

But I observed that with small magnifying power the appearances of the brightest part of the head maintained always a certain unity, which would not admit great mistakes in the observations. Therefore it seems to me that, unless we suppose considerable and unknown variations in the form of the nucleus, only the difference of appreciation of the point observed can hardly explain such a great, and I say regular, difference between one orbit and another.

I say *regular difference*, because I remark a certain peculiarity.

The first elliptical orbit calculated by Mr. J. C. Chandler, using observations from September 18 to October 20, gave a period of about 4000 years.

Afterwards Mr. Kreutz, using observations from September 8 to November 14, gave a period of 843 years, and lately Mr. Morrison, keeping observations from September 19 to December 11, has an elliptical orbit with only 642.5 years.

This fact induces me to believe that an accurate study of the perturbations of the motion of this comet may be as important as it was for Biela's comet.

It is my purpose to go, as far as I can, through a complete discussion of all the observations, and I shall be very glad if those of your readers, who are possessors of good unpublished remarks both about the appearance and about the positions of the comet, would kindly let me know of them.

E. RISTORI

13, Pembridge Crescent, Bayswater, W., January 30

The Aurora of November 17, 1882

I SHOULD like to ask H. J. H. Groneman whether he tried to find out if a curved path for the auroral beam would agree better with the observations than a straight one; because, if it was purely an auroral phenomenon, we should naturally expect its path to be a curve, maintaining a uniform height above the surface of the earth, and to be approximately a small circle having its centre at the magnetic pole, this being the ordinary position of the auroral arches. Of course the motion of the parts of the arch is often not exactly in this direction, because the arch has frequently a transverse motion in addition to the movements that take place longitudinally; and if there was any such transverse motion in the case of this beam, that would prevent its moving strictly along a parallel of magnetic latitude, though it is hardly likely it would deviate far from it. It would be well to ascertain whether such a motion would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was in a straight line; for the establishment of a curved motion would do away with the idea that the phenomenon was caused by a meteor.

In the other cases cited by Dr. Groneman of supposed meteoric masses passing through our atmosphere and producing auroral effects, the paths, so far as given, seem all to have been approximately along the parallels of magnetic latitude, which circumstance militates against their having had anything to do with meteors, because these traverse the atmosphere in all directions, and would be just as likely to go in a northerly or southerly direction as in an easterly or westerly one. Possibly, however, Dr. Groneman's theory may be that meteors only produce an auroral effect when they happen to go in such directions as may be calculated to produce it.

Sunderland, January 29

THOS. WM. BACKHOUSE

As Dr. Groneman in his most interesting paper on the phenomenon of November 17 asks for my authority for the Swedish observation, I may say that I merely saw it in the "Notes" in NATURE (vol. xxvii. p. 113). There seems a misprint in that statement, however, as "Eskibstuna, fifty-four miles south of Stockholm" would be in the sea, whereas Eskibstuna is fifty-four miles west of Stockholm.

As the spectroscopic observation is said to put the auroral nature of the "spindle" beyond doubt, I would observe that until we know that gas excited by the passage of particles through it at fifteen miles a second does not give the same spectrum as when incandescent by an electric discharge, the observation of certain lines cannot prove anything of the exciting cause. Further, a good deal of the light might be reflected sunlight, as that would be scattered over the whole spectrum, and would thus be masked by the faint diffused spectrum of the moonlight at the time.

Bromley, Kent

W. M. F. PETRIE

REFERRING to Dr. Groneman's communication, possibly it may be of service to say that at 9 p.m., October 14, 1870, besides some ruddy auroræ, chiefly in the west and north, I saw a band having a very close resemblance to that figured in the illustration, p. 297. It, however, stretched all the way across the sky from west to east, and continued for some time without much apparent alteration in figure or locality. An appointment called me away before it had vanished.

Cambuslang, January 26

HENRY MUIRHEAD

The Sea Serpent

I HAVE seen four or five times something like what your correspondent describes and figures, at Llandudno, crossing from the Little Ormes head across the bay, and have no doubt whatever that the phenomenon was simply a shoal of porpoises. I never, however, saw the head your correspondent gives, but in other respects what I have seen was exactly the same; the motions of porpoises might easily be taken for those of a serpent; once I saw them from the top of the Little Orme, they came very near the base of the rock, and kept the line nearly half across the bay.

JOSEPH SIDEBOTHAM

Erlesdene, Bowdon, January 26

Influence of "Environment" upon Plants

REFERRING to Prof. Thiselton Dyer's letter on the above subject in NATURE (vol. xxvii. p. 82), it may interest your readers to know that I have had several trees of *Acacia dealbata* 30 feet high, in the open air, in flower for ten days past, but not so fully as they will be in a fortnight's time. I have had *Desfontainia spinosa* in flower during the past eight months; this shrub is 6½ feet high, and all in the open air.

Rosehill, Falmouth, January 29

HOWARD FOX

THE PEAK OF TENERIFFE ACTIVE AGAIN

A PRIVATE letter which I have just been privileged to see, from a native lady in Santa Cruz to her sister in this country, tells how the inhabitants of that present capital of Teneriffe had remarked for several months past, that there was no snow on the upper part of the Peak; though all the "Cumbree," or moderately high land over the rest of the island, was whitened with it in the usual manner for the season. But within the past few days, "fire, like three great bonfires" had been seen on the summit of the Peak, and a lava stream had begun to flow down from it.

Now this is interesting both chronologically, and chorographically. Chronologically, I had remarked at p. 150 of my little book "Teneriffe an Astronomer's Experiment," (published in 1858), that the lava eruptions there only break out about once in a century; the last eruption having occurred in 1798, and the previous one in 1703; and now we have one in 1883, but in what part of the mountainous island called Teneriffe has this last eruption appeared?

So far as I can gather from the said private letter, it has issued, if not from the very mouth of the craterlet which forms the tip-top of the Peak, yet from its sides or foot where it stands on a filled up crater of much larger size, otherwise to be looked on as the Peak's proper and effective summit; and it is from that crater's lips that have proceeded all the later, and yet prehistoric, streams of black lava, which score and frill the Peak on every side; and contrast so strikingly with the far more ancient red, and the still more ancient, more abundant, and once hotter yellow streams from the older and larger craters lower down, before ever yet, the Peak, or final cinder heap, was formed.

But though in the Nature-primeval history of the Mountain, the black, unoxysided lava streams of the Peak, were its latest exudations, still nothing more of that kind was locally expected to occur there within the human period. This was partly because no addition to them had been made since the Spanish Conquest; and partly because the lava outflow of 1798 avoided the Peak, and broke out on the Western side of the general mountain mass, while the eruptions of 1703, which threatened the town of Guimar to the south, and destroyed Garachico to the north, filling up its once beautiful bay—broke forth nearer the sea-level than the peak's top. Whence the idea arose, that the central vent of the peak must have clogged up with time, and that nothing more than its merry little jets of steam and sulphurous acid were to be looked for in that quarter; yet now we are told of red hot lava pouring forth.